

With regard to those exceptions, it may be worth noting that the two in the minimum series of groups (viz. the group for 1856, and that for 1890) occur just about the middle of dry periods in the 35 years' cycle of weather.

Now, the *régime* of rainfall is often different in the north and south of this island. A year that is wet in London may be dry in the north, and *vice versa*. The following figures may throw light on the extent of this.

Compare the annual rainfall at Greenwich, from 1841 to 1897, with that at Rothsay (N.B.), calling each year with excess of rainfall *wet*, and each with deficiency *dry*.

We find a distribution as follows:—

Gr. wet. } 11 Gr. dry. } 19 Gr. wet. } 14 Gr. dry. } 13
Roth. wet. } 11 Roth. dry. } 19 Roth. wet. } 14 Roth. dry. } 13

Thus the rainfall has been opposite in character in 27 out of those 57 years (nearly one-half).

Now, with reference to the sun-spot cycle, it would appear that at Rothsay, it is the years about *maxima* that tend to be the wetter. I am not at present in a position to treat the Rothsay data as those for Greenwich have been treated above. But let us take the annual rainfall, marking each year as + or −, as that has been above or below the average. Then, taking five-year groups about maxima or minima since the beginning of this century, we get the following tables:—

Max.	5-year groups.	Wet.	Dry.
1. 1804 ...	1802-1806	2	3 e.
2. 1816 ...	1814-1818	3	2
3. 1830 ...	1828-1832	3	2
4. 1837 ...	1835-1839	3	2
5. 1848 ...	1846-1850	4	1
6. 1860 ...	1858-1862	3	2
7. 1870 ...	1868-1872	3	2
8. 1884 ...	1882-1886	3	2
9. 1894 ...	1892-1896	2	3 e ?

Sums. ... 26 19

Min.	5-year groups.	Wet.	Dry.
1. 1810 ...	1808-1812	2	3
2. 1823 ...	1821-1825	1	4
3. 1833 ...	1831-1835	2	3
4. 1843 ...	1841-1845	2	3
5. 1856 ...	1854-1858	1	4
6. 1867 ...	1865-1869	2	3
7. 1879 ...	1877-1881	1	4
8. 1890 ...	1888-1892	2	3

13 27

Thus, in the former case (nine max. groups) there is one valid exception¹ to the rule of a preponderance of wet years; while the eight minimum groups show throughout a preponderance of dry years.

The same thing might be shown with more or less distinctness for other stations in the north.

In the eyes of some, a contrariety in rainfall, like that just indicated, doubtless seems fatal to the idea of sunspot influence. But the best thought in meteorology to-day, if I mistake not, would hesitate to affirm that it is so. The same influence, indeed, acting under different conditions, may produce different and even opposite effects.

Discussing the question of sunspots and temperature in his recent admirable "*Traité élémentaire de Météorologie*" (1899), M. Alfred Angot points out that the relation is probably a complex one. "On pourrait concevoir," he proceeds, "que les taches solaires influent, par exemple, sur la position des centres de hautes et de basses pressions, et il serait alors facile de comprendre que ces déplacements produisissent des variations de température d'un certain sens dans une région, d'un sens opposé dans un autre, et nulles enfin dans d'autres encore. Il est possible qu'en reprenant ces études dans cet ordre d'idées on arrive à concilier les résultats, contradictoires en apparence, que l'on a obtenus jusqu'ici." The same thing should obviously apply to rainfall.

Is there any evidence that rain-bringing depressions take, on the whole, a more northerly path across this island (in going E. or N.E.) about the time of maxima sunspots, and a more southerly path about minima? Perhaps something of this kind might account for the oppositeness in rainfall we have considered.

¹ The "homogeneity" of the record before and after 1890 seems a little doubtful.

My object in this letter, however, is rather to call attention to facts than to affirm this or that influence as accounting for them, and it is possible that further inquiry might dissipate the notion that sunspot influence is concerned in those phenomena.

ALEX. B. MACDOWALL.

Periodic Tides.

UNDER the above heading, Prof. A. W. Duff, in *NATURE* of January 12, describes the character of the periodic tides, or secondary tidal undulations, on the eastern coasts of Canada, and offers an explanation for these based upon observations in the Bay of Fundy. Without entering into any discussion of the explanation suggested, I wish to point out that the basis of fact upon which he rests his descriptions for most of the places named, is so meagre and insufficient as to render these descriptions quite misleading.

Prof. Duff has visited St. John in the Bay of Fundy, and has had access to the tide gauge there established by the Tidal Survey. The observations at Quaco and on the St. John river are his own. But with regard to the other places mentioned, his descriptions of the character of the oscillations are entirely based upon a few examples of simultaneous tides, in sets of only four days each, published to illustrate a paper of mine in the *Transactions* of the Royal Society of Canada, and reproduced in one of the reports of progress of this Survey. These were selected to show the character of the main tides, without respect to the minor undulations. On one illustration it was even noted that the tides there represented were unusually free from secondary undulations which appear as a rule at that station. These few illustrations cannot, therefore, properly be employed as a basis for a comparative description of the usual character of these undulations; and it is not right to make this use of them when valuable continuous records from recording gauges exist, by which the subject could be investigated thoroughly as it deserves.

The other places outside the Bay of Fundy named by Prof. Duff are all tidal stations of this Survey, which is being carried on by the Department of Marine, under my direction. There are eight principal stations in the region, extending from Quebec to Halifax, and from Yarmouth at the south end of Nova Scotia to the Strait of Belle Isle. In this region, which includes the Gulf of St. Lawrence and the Bay of Fundy, the tides range from the highest in the world to so flat a tide as to be almost inappreciable except at the springs. At these stations, from two to four years of continuous tidal record has already been obtained, accompanied by meteorological returns from ten stations in the region, and supplemented by a complete file of daily weather charts showing the isobars, issued by the Meteorological Service, which is another branch of the Marine Department. At three of the tidal stations themselves a barograph record is also secured. The monthly charts issued by the Hydrographic Office of the United States, which show the tracks of all the important storms, are also available for purposes of comparison. In addition to these principal stations, there are now fifteen secondary stations, which have been in operation for three or four months during the summer season in the Gulf of St. Lawrence and the Bay of Fundy, at which simultaneous tidal comparisons have been obtained on recording gauges.

At some of these stations the secondary undulations are persistent and continuous; at others, they appear about half the time; while at others again, usually towards the head of estuaries, they seldom (if ever) appear.

The excellent field thus afforded for the investigation of the question was pointed out in a "Note" on the subject, communicated by me, in May 1895, to the Royal Society of Canada (*Trans. Roy. Soc. of Canada*, vol. i., Second Series, 1895-96). The question of the origin of these undulations has been examined by Mr. F. Napier Denison, of the Meteorological staff; his endeavour being to establish a relation with the fluctuations of the barometer. With this in view, he has examined some part of the tidal record as above, and also the tidal record from our Pacific coast stations. The results of his investigations, as far as he has yet carried them, are published in the *Proceedings* of the Canadian Institute, Toronto; paper read January 16, 1897. He has also made an investigation of similar short-period undulations in the Great Lakes (see *Proceedings Canadian Institute*; paper read February 6, 1897). This, with the references given by Prof. Duff to his own papers, completes the study yet given to the subject in Canada.

It is unlikely that better conditions exist anywhere than in the region above referred to. Some of the waters are land-locked, some open to the ocean; the great variety in the range of the tide, tending to magnify the undulations where the range is great, and leaving scarcely anything else but these undulations and the effect of storm disturbance, where the tide is flat; the completeness of the meteorological data and the well-charted storm tracks, furnish ample material for comparison. The investigation has not yet been taken up by this Survey, which has to be carried on with so little means and assistance as to confine it at present to the direct practical issues in the preparation of tide-tables, &c. But where such good material exists, it is very unfortunate that descriptions of the phenomena from a few illustrations should be given as an average account of their characteristics, or that conclusions should be founded upon too narrow and incomplete a basis.

W. BELL DAWSON,
Engineer-in-Charge of Tidal Survey.

Ottawa, February 10.

MR. DAWSON characterises my letter as "misleading," and yet, in the course of his own letter, quite neglects to point to an incorrect statement in mine. This is certainly unfortunate.

To show how little Mr. Dawson's remarks touch the substance of my letter, permit me to briefly re-state my position. (1) The oscillations are regular where the basin is fairly regular. This is not questioned by Mr. Dawson, and, as regards the Bay of Fundy, it is amply confirmed by my own observations and the records of Mr. Dawson's department. (2) The oscillations are of irregular period in markedly irregular basins, such as the Gulf of St. Lawrence. This is also not questioned by Mr. Dawson. It is founded on records of four days each from seven different points on the Gulf of St. Lawrence (see the Tidal Report referred to by Mr. Dawson and quoted in my previous letter). Mr. Dawson's only criticism is that he has many other records from the same places; but he does not tell us whether they contradict the published ones. It would certainly be surprising if they did. (3) The period is determined by the dimensions of the basin, and can be calculated from those dimensions, as I have tried to show. (4) The cause of the initial disturbance is probably atmospheric. This point is discussed by Mr. Napier Denison in a short but valuable paper that reached me after my first letter was published. Mr. Denison confines his remarks to the cause of the initial disturbance.

That the period of these oscillations should be determined by the atmosphere seems to me quite incredible. It is surely sufficient refutation that, within a radius of twenty miles from St. John, we have three points at which the regular periods are 35 seconds, 12½ minutes, and 43 minutes respectively, and at one of these points the 35-second and 43-minute oscillations coexist.

Perhaps I have misunderstood Mr. Dawson. If his purpose was to call attention to the valuable materials being gathered by the Canadian Tidal Survey, which Mr. Dawson directs, then I must express my hearty approval, and add the hope that the excellent work may continue and receive efficient support. May I add that my interest is not that of a casual visitor to St. John (as implied by Mr. Dawson), but of a Canadian, most of whose life was passed in St. John? A. WILMER DUFF.

Purdue University, Lafayette, Ind., U.S.A.

The Natural Prey of the Lion.

JEAN BAPTISTE TAVERNIER, in his "Travels in India" (translated by V. Ball, 1889, vol. ii. p. 397), mentions a case similar to what Mr. Crawshaw describes under this heading in your last number (p. 558). "At a distance of two or three leagues from the fort [at the Cape], the Dutch found a dead lion which had four porcupine's quills in its body which had penetrated the flesh three-fourths of their length. It was accordingly concluded that the porcupine had killed the lion. The skin is still kept with the spines sticking in the foot." Thereon it is noted by the English translator that "numerous cases are recorded of tigers having died in India from this cause, and also of occasionally having been found when shot to have porcupine's quills sticking in them." The old Chinese motto, "*the hedgehog defeats the tiger*, and the serpent stops the leopard" (in Liu Ngan, "Hwui-nan-tsze," second century B.C.), is probably founded on observations allied to these. KUMAGUSU MINAKATA.

7 Effie Road, Walham Green, S.W., April 15.

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THE PRESENT STANDPOINT IN SPECTRUM ANALYSIS.

IN a former article I referred to some of the difficulties encountered by the earlier researchers in spectrum analysis. In the present one I propose to pass over the history of nearly twenty years' work with all its attendant doubts and difficulties, and deal with what that work has brought us, a perfect harmony between laboratory, solar and stellar phenomena.

It has been proved beyond all question that not only are both fluted (or channelled-space) spectra and line spectra visible in the case of most of the elements, but that many of the metallic elements with which I shall have to deal in the sequel have at least two sets of lines accompanying, if not resulting from, the action of widely differing temperatures.

It is important to mention that the different chemical elements behave very differently in regard to the action of heat and electricity upon them as we pass from the solid to the liquid and vaporous forms; that is, the two different forms of energy are apt to behave very differently, the permanent gases as opposed to the elements which generally exist in the solid form is the first differentiation, the elements of low atomic weights and low melting point as opposed to the rest, is the second.

In the cases in which heat-energy can go so far, we first get an increase in the free path of the molecules, and ultimately the latter are made to vibrate.

In the case of electricity, on the other hand, increase of free path is scarcely involved, and hence we may have effects similar to those produced by high temperature, with scarcely perceptible effects of heat in the ordinary sense.

Conversing on this subject with my friend Clifford, many years ago, we came to the conclusion that the energy imparted to a molecule might cause (1) an extension of free path; (2) a rotation, and (3) a vibration. To get concrete images of these effects we spoke of *path-heat*, *spin-heat*, and *wobble-heat*. The facts seemed to show that heat energy had no effect in producing line-spectra until the two first results had been obtained, and, further, that in all gases and many metals it had no effect in producing vibrations; while, on the other hand, electrical energy generally acted as if it began at the third stage, and is effective in the case of every chemical substance without exception.

However this may be, we now know that many elements present changes at several widely differing stages of heat. The line spectra of elements like sodium, lithium, and others may be obtained by the heat of the flame of a spirit lamp, or an ordinary Bunsen's burner, the substance being introduced into the flame by a clean platinum wire twisted into a loop at the end.

This temperature has no effect upon iron and similar metals. To get any special spectral indication from them a higher temperature than that of the Bunsen is required, the blowpipe flame may be resorted to; in this a stream of air is blown through the centre of a flame of coal gas burning at the end of a cylindrical tube.

We get in this way what is called a "flame-spectrum," in which flutings and some lines are seen. In order to obtain the complete line-spectra of some of the less volatile metals, like iron and copper, we are driven to use electrical energy and employ the voltaic current, and (for choice) metallic poles which are so strongly heated by the passage of the current that the vapour of the metal thus experimented on is produced and rendered incandescent.

We may say generally that no amount of heat-energy will render visible the spectra of gases. These are obtained by enclosing the gases in glass tubes and illuminating them by means of an electric current. We may go further and say that the ordinary voltaic current